ANALYSIS 8 . 3 JANUARY 1948

IS METAPHYSICS IMPOSSIBLE?

By A. C. EWING

IT is a common view very widely taken for granted nowadays that we cannot by philosophical argument establish any facts over and above the facts already established by our ordinary everyday experience and the natural sciences, and that the differences between philosophers, properly understood, are never really differences about such objective facts but rather about the use of language. This view is often attributed to Professor Wittgenstein, and it has certainly been very commonly asserted by people who had been greatly influenced by him. But as he has denied that his view could be properly thus expressed, the present article must not be taken as a criticism of him. However, whether due to him or not, the view is having a very important influence and deserves discussion on that account.

Now I wish to ask on what grounds this view could possibly be maintained. It is not self-evident. It is not capable of establishment by sense-experience, since sense-experience cannot possibly prove the non-existence of facts beyond sense-experience. It is not formally deducible by logic or mathematical reasoning. It can hardly be conceived capable of establishment by the ordinary methods of the natural sciences as are theories about evolution or the origin of the earth. How then can it be shown to be true? Even an empiricist is not entitled to assert the truth of non-empirical propositions for which he has no evidence. I ask what is the evidence. In doing so I am not putting forward a claim that most affirmative metaphysical propositions (or any particular such propositions) are true; I am merely asking whether we have any justification for dismissing them all indiscriminately or must not rather try to judge between them on their own merits.

It used to be given as a ground for dismissing metaphysics that all unverifiable statements are meaningless. But if we mean by "verifiable" "conclusively verifiable by sense-experience", the assertion can carry no possible weight, for it would be itself

meaningless on its own showing. If, on the other hand, we are using the verification principle in the weaker sense in which it signifies only that a statement is meaningless if human experience is not in some way relevant to it, we are not excluding any metaphysical statements at all except the notorious "ontological proof". I am quite willing to make anybody a present of that proof. But apart from the ontological proof every metaphysical argument that has been put forward by a respectable philosopher has had some empirical premiss, if only that "I exist", and all the systems of metaphysics that have been propounded would be claimed by their authors to be justifiable as the best interpretation of experience. Very likely their authors were wrong—not more than one could have been quite right in any case—but they may have been partly right, and how far they were right or wrong must be decided by considering each case on its own merits. No doubt a metaphysician would usually include in experience more than sense-experience, but to assume prior to detailed investigation that moral experience, religious experience, and the experience of seeing logically necessary connections yield no information, except in so far they can just be reduced to sense-experience, is an *a priori* dogmatism not worthy of the empiricist. Again, it used to be said that metaphysical propositions would have to be synthetic *a priori* and there can be no synthetic *a priori* propositions. But since the proposition that there can be no synthetic *a priori* propositions would itself have to be synthetic *a priori* if it was to be justifiable at all, this argument seems to me suicidal.

However, some people who claim to have got rid of metaphysics with the help of such principles would nowadays deny that they were asserting them as dogmatic propositions. They would say that they were merely methodological assumptions not to be taken literally. To such philosophers I should like to put this question. How can a principle be possibly used as a premiss to establish any conclusion if it is not more than a methodological assumption? If we argue from it and then take the conclusions we get as true, we are assuming that it is a true proposition, not merely a methodological assumption, whatever we may say. For if it is not true, no argument in which it is used as a premiss will carry any weight. Perhaps the reply will be made that the verification principle and the principle that there are not synthetic *a priori* propositions are not true as they stand but are useful assumptions because they come near the truth; but in that case it is surely most desirable to try to state what exactly the truth is to which they come near and not merely to

reiterate the methodological assumption as a slogan. Further, one would still have to prove that they did come near the truth, and I should very much like to know whether anybody can produce such a proof. I have never seen one yet, and in its absence I do not see how we could be justified even in accepting them as methodological assumptions.

It is sometimes said that, though the impossibility or meaninglessness of metaphysics cannot be proved, it is something which one comes to *see* when one has had a sufficiently long course of treatment by some of my friends at Cambridge. If it is meant that this is a truth which can be known by an *a priori* intuition which people have at the conclusion of this course of treatment, I cannot disprove their intuitions any more than they can disprove the intuitions of metaphysicians, which are also often claimed to seem evident after a long course of training and discipline in philosophy, though one of a very different kind; but I think that what would rather be said is not that they see intuitively that there cannot be metaphysics but only that after a proper course of training they just cease to be puzzled by the alleged metaphysical problems by which they used to be puzzled. But there are other means of escaping from puzzlement. Taking drugs is one, simply not thinking another. Again well-trained and enthusiastic Nazis and well-trained and enthusiastic supporters of any dogmatic religion no doubt ceased to be puzzled about a great many things as a result of their training, but does that of itself prove that they were right in thinking them solved? Perhaps they would have been in a more reasonable intellectual condition if they had still been puzzled by them. I have heard it admitted by adherents of this view as a "difficulty" that puzzlement might also be removed by drugs, but I think they take the difficulty altogether too lightly. For till the difficulty has been removed there can be no justification for their view, because till then there is no evidence that they have effected the removal of the puzzle in the right way and not in the wrong way. A problem is not solved by the mere fact that one is not puzzled by it; if it were, the golden age of philosophy would lie in the time when men were not developed enough even to think about it. Of course, if a definite solution of a particular problem is given, that is another matter which must be debated on its own merits; but the trouble, it seems to me, is that most of the solutions of particular problems reached by this method will not convince anybody who does not already assume that there are no facts over and above facts which could on principle be established by ordinary empirical tests of a non-philosophical

type and who does not therefore already assume that any words which seem to imply the contrary can only be used intelligibly if this implication is cut out. Thus it is often said that the different theories of perception, since the decision between them cannot be made by empirical means, merely assert the same facts in different language. But I cannot imagine that anybody would think this who did not first assume that there were no facts which could not be on principle established empirically: it is only if we first assume such a view and deduce that the different theories therefore can differ only linguistically that we shall come to the belief that the difference is only linguistic. On a straightforward inspection of the theories it seems quite obvious that they have a different meaning, and it is only when one has been indoctrinated with the verification method that the opposite seems to be true, though of course it is impossible to express what the difference is if one is confined to the empiricist language of the verification principle. If to explain the difference we have to say what difference it would make to sense-experience if one theory were true rather than another, of course we cannot do it; but to say that this proves there is not a difference is to beg the whole question by assuming at the start that any difference there is between two theories must be expressible in terms of sense-experience.

In the long run I do not think that the modern movement in philosophy, though it may have the credit of removing some particular misunderstandings of language which led to mistaken or unfounded metaphysical assertions, has brought any fresh argument of weight against metaphysics in general. There remains the old, old—but still very formidable—argument that metaphysicians are always in hopeless disagreement. Here I owe a debt to the modern philosophers I have just been criticising. They have themselves provided some suggestions which I propose to steal in order to use for the defence of metaphysics. They have suggested that, when two or more philosophers appear to be contradicting each other, what really happens is this. Each philosopher uses a different word, neither word is quite appropriate, but each (if the philosophers are competent) conveys valuable suggestions, and what they call the arguments for their different theories are statements of the respects in which their words are appropriate. The arguments of both sides may thus express truths in so far as each word is really appropriate in certain respects—and inappropriate in others—and each side may render a service by calling attention to the facts which make their word in some degree appropriate and suggestive. If we

combine the two sets of arguments (in so far as these are not just muddled) we shall have, not indeed an authoritative decision that one word is right and the other wrong, but a much better account than we could have got from either side alone of the sense in which either word is applicable. I say "either word", for it will not now matter much which we use provided we make clear all the reservations needed which the arguments for the other side have given us. This suggestion has been brought forward in connection with problems of what would be called critical philosophy rather than metaphysics. But it may also be applied to disagreements in metaphysics, with still greater plausibility, since we are more likely there than in merely analytic philosophy to suffer from the disability of just not having the right words to describe the facts as we see them. And it would thus remove the objection that metaphysics must be abandoned because philosophers perpetually disagree. They may disagree, and yet each side may in its partial truths have something very valuable to say. I think there is a great deal in this, though I certainly do not hold that it covers all metaphysical differences.

Finally, I should suggest that the very critics of metaphysics cannot get on without presupposing it. I am not indeed here thinking of a metaphysics supposed to consist in the study of some other more ultimate world quite different from the world of our experience, but of a metaphysics to which our experience leads us and which can be described in terms drawn from our experience, though we certainly should not limit experience to sense-experience. Nor am I contending that it is possible for the human mind to produce an elaborate detailed metaphysical system, as Hegel or McTaggart thought they could, which is true and valid. I think whether this is so or not can only be settled *ambulando*. But if we define "critical philosophy" as the study of common-sense beliefs and concepts and "speculative philosophy" or "metaphysics" as the study of the general nature of what exists, we can say that there is no real dividing-line between critical and speculative philosophy. At least the only way to establish such a dividing-line would be to take the common-sense beliefs studied as completely false or unjustified. In that case indeed their study would not be a study of metaphysics. But if the common-sense beliefs are taken as true, or in so far as they are taken as true, to give an analysis of them is, if the analysis is correct, to assert at the same time propositions about the general nature of what exists, thus conforming to the definition of metaphysics I have just given. Thus to analyse our common-sense propositions *e.g.* about matter is at the same

time to advance a metaphysical theory about matter, at least if the common-sense propositions are taken as true. And, even if they are held to be not wholly but partly true, to analyse them is to make at any rate partly true assertions about the real, and the process of analysis can hardly be separated from the sorting-out of those parts of them which are true. And I should ask the philosopher who does not make metaphysical statements, but still *qua* philosopher prefers one mode of linguistic expression to another on the ground that it is more illuminating, though it does not state new empirical facts, to explain why he thinks it more illuminating if it is not because it gives more truth or comes nearer to giving him truth about reality.

TRUTH AND KNOWLEDGE

Some Comments on Russell

By Karl Britton

1. In the introduction to the *Inquiry into Meaning and Truth*, Russell sketches two sorts of theory of knowledge. The first is purely psychological-social, and deals with believing as a natural process : it refers to habit, learning, and "appropriate responses". The second is, in effect, the Cartesian inquiry into what sorts of belief are most reliable ; what are the tests that must be applied to the different sorts of belief. It is a " critical scrutiny of what passes for knowledge ", and " truth " is its fundamental notion. One of its principal aims is to reveal the basis of Knowledge (in the strictest sense of the word) upon which our beliefs rest. It is this second inquiry, into questions of validity, that Russell calls " epistemology " and which he pursues in this book.

The point of the distinction is to suggest that we cannot, by analysing the causes or consequences of a given belief, determine whether it is valid or not. In my opinion, this is the case. But there are, of course, several contemporary schools of philosophy which attempt to determine questions of validity by a purely *causal* analysis. It is, for example, true of John Dewey that he tries to show that the truth (or " warranted assertibility ") of a belief can be assessed by references to its causal *consequences*. And this doctrine of consequences has been attacked by Russell again and again during recent years. But it is interesting to note that, in the *Inquiry* itself, Russell appears to put forward the view that the true belief is to be distinguished from the false by its *causes*. Indeed, he goes so far as to say that the main difference between himself and Dewey is simply that he defines a true belief by reference to its causes, and Dewey by reference to its consequences. (*History of Western Philosophy*, p. 853.) I shall try to show that this handy description is far from just to Russell himself : for Russell is concerned with causes which are *perceived*, and which, as additional data, are supposed to give *reasons* (as distinct from causes) for the belief concerned. In this way he passes from psychology to logic : and my purpose in this paper is to uphold the view that an account of " Truth " and " Knowledge " cannot be given in causal or empirical terms, but requires reference to procedures or rules—*i.e.* to considerations quite different from habits and outside the scope of psychology altogether. The question, in the first instance, concerns the simplest types of propositions.

2. In his recent writings, Russell sets out to find those statements which I believe on the evidence of a single experience, and which I cannot afterwards be made to doubt. Here "cannot" is supposed to stand for *psychological* impossibility, and he admits, of course, that this is in itself no mark of truth or even reliability. But he holds that an important group of statements which meet the two requirements are of the form " This is red ", and that we have in fact some *reason* for believing them—although he sometimes says that the reason is not always conclusive. The reason given is the surprising one that in such cases I directly *perceive* a quasi-causal relation between an experience and my own uttering of the sentence " This is red ". My statement then " corresponds " to the fact in a sense of " correspondence " which implies no more than reference to an external, non-linguistic event. And the belief will be an instance of " Knowledge ", or the nearest thing that we can ever achieve to an instance of Knowledge.

I do not feel disturbed by the notion that I may perceive a causal relation, provided we admit that " cause " is a *family-word* in the sense in which Russell (following Wittgenstein) says that " word " is a *family-word* (*Inquiry*, Ch. I.). At all events, I can see the point of saying that we sometimes know a cause of one of our own actions, without " coming to know it " by repeated experiment and inductive inference. But there is another, and a grave difficulty in this view, as Russell is fully aware. It is that, in describing merely what I observe with my senses on a given occasion, I am making a statement which does not logically allow of falsehood. For if I say " brown " instead of " red ", using " brown " to describe what I in fact see, then the only objection that could be made to my statement is that it is socially incorrect, *i.e.*, at variance with a widely-followed rule. It may be inapt but it is not false. It is worth noticing, since Russell is so often concerned with a Robinson Crusoe talking to himself, that even such a communication could still be called " inapt " or " meaningless ". For we can suppose a language governed by rules previously formulated, even though there is only one person who has ever used the language : and in that case Crusoe might catch himself asking : " Was I correct in describing that as ' brown '? "—*i.e.*, " Did I follow *my own rule* here ? "

Supposing this were to happen, would it reassure him to think that he perceived a causal relation between what he saw and what he uttered ? Russell supposes it would : the perceived cause would give him reason to think that he was indeed

applying the rule he had formulated—that he was using his words *rigidly*.

But on reflection, I think Russell is mistaken here. Surely the perceived cause would give him reason to believe that he was acting in accordance with *habit*. But of course his habit might be wrong. We sometimes have the experience of performing a habitual action and noticing that this is *not* what we were originally taught to do—that the habit does not square with the rule. So that Crusoe might perfectly well note that a statement was made incorrectly, or doubt whether or not it was correct, although he could *perceive* the operation of habit in causing him to utter the statement.

So that, while I admit that Crusoe might know that a given experience (say S), caused him to utter the expression " S ", the only *useful* knowledge he could have here, would be that in fact S is, or is not, correctly described as " S ". This knowledge arises when he *adjudicates* upon S, under the rule which he has learned or made, and decides whether or not S comes under the rule about " S ".

To describe correctly means in accordance with a rule. There are, therefore, procedures of description as well as of inference. In the case of the former, the rule applies to a non-linguistic event and it follows that only an observer can apply the rule ; whereas in the latter case, the rule applies to a statement and is a form of " pure calculation ". It is notable that Russell many times writes as though the relation of " basic " propositions to experience can be dealt with in purely psychological terms (" what I see makes me say ' red ' "), and that only inferential relations lead us into question of validity. (See *Inquiry*, p. 18.) I think this is an important mistake.

3. The discussion so far is about whether a given expression makes sense or is nonsense : not whether it is true or false. In order to find an instance of an expression that *can* logically be true or false, we must introduce some element of forecasting or prediction. This is very often said by Russell himself. Let me construct an example. Looking at the evening sky I say : " Tomorrow will be fine ". This statement can (logically) be true or false : both " fine " and " not fine " make sense here. We could interpret the statement: " The sky is red " as a mere description of what I now see : that is, as a statement that cannot be true or false. And when tomorrow comes, " It's raining " could be understood in the same way. Moreover, when, tomorrow, I argue that " ' Tomorrow will be fine ' was false " in view of the fact that " It's raining " is now true, then I am

making a pure deduction and no *other* conclusion will make sense. But the forecast itself has the two meaningful possibilities: it may be true and it may be false.

Can we be said to know such forecasts ? If we ever do (I think Russell would hold), then it must be in virtue of some inferential connection between the forecast-statements and those logically incorrigible statements in which we describe what we now perceive. Thus if I can know that " Tomorrow will be fine ", it is because I now know that the sky I see is correctly described as " red ", and know that the two statements " The sky is red " and " Tomorrow will be fine " are correctly described as instances of a certain inferential relationship.

So that if ever I know a true-or-false proposition, it is in virtue of applying certain rules, not in virtue of perceiving causes. I must first " know the evidence ", i.e., describe it according to rule ; and I must know the syntactical connection of this description with the proposition that I pretend to know. And if at any stage I question, my doubt can be set at rest (if at all) by re-applying the same rule or applying the rules which (as I calculate) are the equivalent. And if at any stage I find that the results I get are not the same as the results which " all the others " get, then the only thing that can lead me to set aside the ruling I made will be my own interpretation of what I observe them doing ; *i.e.*, another statement which I make by reason of my application of other rules of description and of inference to what I myself observe of their behaviour.

4. Whether or not my description of what I see is my habitual response, or is the response generally made by members of my community, or is a socially useful response, or an " appropriate " response—all these are general empirical questions to be settled in the same sort of way as questions about rats in mazes. But they do not settle the question of validity—" Do I know or don't I ? " This question has to be settled by my asking myself : " Is what I see correctly described as ' red ' ? " *i.e.*, " Does this follow from the rule of description ? " Similarly, " Do I know that $5 \times 237 = 1185$? ", is answered by re-applying the same (or some equivalent) rule of inference to the expression " 5×237 ".

There are Empiricists who would see here an element of dogmatism. They would prefer to solve The Problem of Truth by finding *a scientific description of the general properties of all true beliefs*. In my view the phrase " The Problem of Truth " is itself misleading. For the rule by which a belief is tested is not an omnicompetent rule, but simply the rule about " red " or the rule about " 5×237 ". While Russell often insists most clearly

on the " autonomy " of logic, he has made many unnecessary concessions to the psychologisers. And it is in this light that I view his doctrine of distinguishing true elementary propositions from false by reference to their causes.

University of Wales.

CONDITIONS FOR AN EXTENSIONAL ELEMENTARISTIC LANGUAGE

By GUSTAV BERGMANN

1. The expressive possibilities of extensional languages have been studied by philosophical analysts for some time. Since the question I propose to discuss is part of this larger issue, I shall begin by restating a familiar syntactical definition of extensionality. A symbolic language S is called extensional if formal equivalence between any two of its predicates implies the equivalence of any two expressions, the second of which is obtained from the first by replacing all or some occurrences of one of the two predicates by the other. Taking non-relational predicates of the first type one has: If S is extensional, then

(1) $\quad f(x) \equiv_x g(x) \;.\; \supset \ldots . f \ldots . \equiv \ldots . g \ldots .$

is a theorem (tautology). The dot patterns with 'f' and 'g' are used to represent the two expressions.

2. Consider now the problem of constructing a symbolic language S_1 that is adequate for speaking about a certain area, say, for instance, a fragment of the world of tones as we know it with respect to pitch and loudness. To explain what is meant by 'adequate', I shall say that S_1 is required to contain (the equivalents of) such statements as 'This is a middle c', 'This is higher in pitch than that', 'Middle c is a pitch, not a loudness'; and, also, such statements as 'This (quality) is a loudness'. The occurrence of 'quality' in the sentence last mentioned is due to the scarcity of idiomatic terms for the qualities within the dimension of loudness; 'soft is a loudness' would be an idiomatic equivalent, though admittedly a poor one, of what S_1 is supposed to contain. But it is not supposed to contain (the equivalent of) such difficult terms as 'quality' whose status in a symbolic language is a major problem in itself, except in such clauses as '$(f) \ldots f \ldots$' and '$(\exists f) \ldots f \ldots$', which are customarily transcribed by 'for all qualities so that . . .' and 'there is a quality so that . . .'

It seems natural to attempt the construction of S_1 by adding to the (logical) formalism of the functional calculus a class (not series!) of undefined non-relational predicates for pitch ('p_1', 'p_2', . . .), another such class for loudness ('l_1', 'l_2', . . .), two undefined relational predicates ('ld', 'hg') corresponding to 'louder' and 'higher-in-pitch' and, finally, two more predicates, 'P' and 'L', for 'pitch' and 'loudness'. A number of particulars ('a', 'b', ...) must also be made available. Such

formulae as '$p_1(a)$', '$\mathrm{ld}(a, b)$', '$P(p_1) \,.\, \sim L(p_1)$' will then represent such statements as 'This is a middle c', 'This is louder than that', 'Middle c is a pitch, not a loudness'. From this it will be seen that in S_1 all the undefined predicates, *except* 'P' and 'L', are of the first type. 'P' and 'L' are of the second type.

3. I am ready to state the question with which this paper is concerned. But before stating it, I wish to make it clear that I do not, in this paper, intend to contribute arguments in support of either an affirmative or a negative answer. I merely propose to show how the answer to *this* question depends on the answers to two *other* questions. And now for the question itself. *Is it possible to define, within S_1, two second-type predicates '$\bar{P}$' and '$\bar{L}$' so that (a) their definientia contain only the undefined predicates of the first type, and (b) '$\bar{P}$' and '$\bar{L}$' can be proved to be formally equivalent to 'P' and 'L' from premises that correspond to true (factual) statements about tones and themselves contain no other undefined predicates than those of the first type*? The statement of the transitivity of 'louder', '$\mathrm{ld}(x, y) \,.\, \mathrm{ld}(y, z) \supset_{xyz} \mathrm{ld}(x, z)$' may serve to exemplify the form of the expressions admitted as definientia by (*a*) and as premises by (*b*). It will be seen that they belong to S_2, where S_2 is the *lower* functional calculus, supplemented by some descriptive terms and, if needed, by quantification with respect to its predicate variables. The definitions mentioned in (*a*) will, of course, be definitions in use. The affirmative answer to the question I have asked may be called the thesis of *elementarism* or, more precisely, elementarism with respect to the area which S_1 is required to describe adequately. If the qualification concerning the scope of S_1 is tentatively omitted, the choice of the name 'elementarism' will, I believe, seem reasonable. For if it is thus generalized, the thesis asserts—somewhat vaguely but, I presume, intelligibly—that in a symbolic language that is "adequate for all purposes" definitions that fulfil conditions corresponding to (*a*) and (*b*) can be devised for all undefined predicates of all types but the first.

Some may agree that a certain symbolic language, such as S_1, is "adequate" for a certain area and, also, that the thesis of elementarism holds for it, and yet may refuse to admit that the symbolism (S_2) obtained by omitting (from S_1) the undefined predicates of the higher types is "adequate" in the same sense. The probable reason for this refusal is twofold. First, the defined predicates of the higher types, such as '$\bar{P}$' and '$\bar{L}$', are merely formally equivalent to the undefined predicates, such as 'P' and 'L', which they are supposed to 'replace'. Second, these

formal equivalences are the consequences of factual statements, namely, the statements referred to as premises in (*b*). The question, or questions, this criticism raises is a *further* question, and one that I do not propose to discuss in this paper.[1] To understand better the sort of thing the thesis of elementarism, as here defined, does assert, it may be helpful to recall one of the possible meanings of the ambiguous term ' nominalism '. For this latter term may be taken to mean that in an adequate language with a logic like (or very similar to) that of the functional calculus all undefined predicates, including those of the first type, can be "replaced by defined terms". Thus construed, the thesis of nominalism is, I take it, patently absurd. But I am, in this paper, not talking about nominalism; I mentioned it only because I wish to elucidate what the sort of thing I am talking about is. To minimize vagueness and to avoid certain complications, I shall limit myself to S_1 and shall not consider the thesis of elementarism in general. The limitation is, perhaps, not as radical as it may appear at first sight. For it seems plausible, to say the least, that if the thesis of elementarism holds in general, it will also hold for S_1.

4. Assume that, *as is actually not the case,* all the tones and only the tones of a certain pitch p_1 have a certain loudness l_1. The formula ' $p_1(x) \equiv_x l_1(x)$ ' of S_1 would then be true (correspond to a true statement). Therefore, if S_1 is an extensional language, the formulae ' $P(p_1)$ ' and ' $P(l_1)$ ' are, according to (1), either both true or both false, and the same holds for the pair ' $L(p_1)$ ', ' $L(l_1)$ '. But we know that, in fact, ' $P(p_1)$ ' and ' $L(l_1)$ ' are both true and that ' $P(l_1)$ ' and ' $L(p_1)$ ' are both false. It follows that, if in S_1 two undefined predicates of the first type are, in fact, formally equivalent, then S_1 cannot be both extensional and adequate, in the sense in which I have used the term ' adequate '. This, I believe, is the point of a very succinct remark by Quine.[2]

5. The lower functional calculus is known to be extensional and so is, therefore, S_2. Any defined predicate, ' $\bar{P}$ ' or ' $\bar{L}$ ', that could be devised in S_1 according to (*a*) and (*b*) would, therefore, be possessed either by both or by neither of the two undefined predicates ' p_1 ' and ' l_1 ' which are, by assumption, formally equivalent. It follows further that, *if* S_1 *is extensional and if the thesis of elementarism is to hold, then it must be possible for*

[1] For a discussion of some of these questions see "Undefined Descriptive Predicates" *Phil. and Phenom. Res.* 8, 55–81, where I have also proposed actual definitions of ' $\bar{P}$ ' and ' $\bar{L}$ '. These definitions contain a syntactical identity; the question of extensionality was thus left open.

[2] p. 127 of "Notes on Existence and Necessity", *Journal of Philosophy*, 1943.

S_1 *to be adequate without containing any two undefined predicates of the first type that are, in fact, formally equivalent.* This and only this is the point I wish to make.

It seems to me that the argument I just proposed could also be made if an undefined predicate of the first type were, in fact, equivalent to the product, or any other compound, of two or more such predicates as in the familiar illustration

$$\text{human}(x) \equiv_x \text{featherless}(x) \,.\, \text{biped}(x),$$

where ' human ', ' featherless ' and ' biped ' are , for the sake of the argument, taken to be undefined. If this is so, as I believe it to be, then the condition for the possibility of an elementaristic and extensional language will have to be modified accordingly. One may further ask whether a similar situation would not arise whenever two compound expressions of the first type are, in fact, formally equivalent as in the illustration

$$\text{rational}(x) \,.\, \text{animal}(x) \equiv_x \text{featherless}(x) \,.\, \text{biped}(x),$$

where all four predicates are, for the sake of the argument, taken to be undefined. I am inclined to agree with those who are of the opinion that this last case would offer no difficulty, or rather, that whatever difficulties it offers are significantly different from the one pointed out with respect to pitch and loudness. As was to be expected, the issue depends in part on what is to be considered an adequate symbolic language. This, to be sure, is a difficult problem and goes far beyond what I proposed to discuss. But, on the other hand, I have no doubt that a language in which one would have to say that one and the same quality is both a pitch and a loudness is not adequate in any sense that is both reasonable and relevant.

A NOTE ON THE ONTOLOGICAL PROOF

By A. M. MACIVER

THE following point arose in one of my tutorials in Leeds in a discussion of the Ontological Proof in Descartes. It is not new—nothing connected with the Ontological Proof is *that*—but we found a way of putting it more simply than I remember ever seeing it put in any of the expositions familiar to philosophers.

The *logical* flaw in the Ontological Proof is obvious (and very familiar). It is an argument from a definition, and such an argument can only yield a hypothetical conclusion. In the proof from the definition of " equilateral triangle " that an equilateral triangle must be equiangular, the conclusion can only mean " If anything *were* an equilateral triangle, it *would* be equiangular ", and similarly in the proof from the definition of " God " that God must exist, the conclusion can only mean " If there were a God, then He would exist ". But it seems that we already know this without the Ontological Proof. Even an atheist would accept it.

But *this* conclusion could be reached concerning *any* subject. What induced men of the intellectual calibre of Anselm and Descartes to think that they had found an argument which applied only to the peculiar case of God?

The answer is that they misinterpreted the proposition " God is a *necessary being*, in the sense that His essence involves existence ". This is a perfectly sound theological proposition (if *any* theological proposition is sound), and it *looks* as if it entailed " God exists ". But it does not.

It means that we can say " There is a God ", and we can say " There is *no* God ", but we can *not* say " There is a God but there might not have been ". All *other* beings are " *contingent* beings "—that is to say, concerning any other X, "X exists" means " There is an X, though there might not have been ", and " X does not exist " means " There is no X, though there might have been "—but, in the case of the propositions " *God* exists " and " *God* does not exist ", no such expansion is possible.

This may be an interesting and important fact about these propositions, although it does not follow (as Anselm and Descartes mistakenly supposed) that " God exists " must be true and " God does not exist " false.

University of Leeds, October, 1947.